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ABSTRACT

Dissatisfied with the linguist's concentration on structure, sociolinguistically inclined linguists turned to anthropology to give an empirical orientation to linguistics. Almost at the same time anthropologists were trying to give a more theoretical orientation to their subject of study, and the structural methods was being sought as the remedy. The structural method, an invention of the linguist, was characterized by a shift from the study of conscious linguistic phenomena to a study of their unconscious infrastructure and from the study of the contingent to the study of the necessary. This shift has now been strengthened by transformational grammar with its realistic (as opposed to instrumentalistic) theory of language. In attempting to extend this framework to the study of sociolinguistics, the views of Itkonen and Derwing are examined and Labov's "socially realistic" method is analyzed. It is concluded that for a genuine sociolinguistic theory to be possible, the Chomskian notion of competence must be borrowed and enlarged in the manner of Hymes. Only an approach of this kind will ensure that the principles of structuralism can apply to the study of sociolinguistics, leading to what Hymes calls "the ethnography of symbolic forms." (Author/NCR)

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THE NATURE OF SOCIOLINGUISTIC INQUIRY

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THE NATURE OF SOCIOLINGUISTIC INQUIRY

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In 1969, De Camp asked the question "Is a sociolinguistic theory possible?" (Alatis 1969), and elicited considerable interest from his audience, among them William Labov. In retrospect, the question appears interesting more for the presuppositions it carries about the practice of sociolinguistics current then than for its intrinsic curiosity or for the conception of theory that De Camp provides as an answer. As for the intrinsic interest, I doubt very much if any sociolinguist today would admit to working without a theory; most would in fact rule out the possibility of a sociolinguistics without a theory. Not only is a sociolinguistic theory possible, they would say, sociolinguistics is not possible without a theory. It is possible that the changed concept of theory (in the sense of an explanatory model) has gained wider popularity since then, but the fact remains, as Labov pointed out in his comments, that De Camp's concept of theory was a very narrow and inadequate one, which may also explain why he thought it fit to ask the question at all. Although De Camp talked of an abstract theoretical approach, raising hopes that sociolinguistics may now be provided with a theory of its own, perhaps even incorporating linguistic theory as a subcomponent, in actual fact De Camp did little more than "reducing data into a form that will fit Chomsky's model" (Labov's comments on De Camp 1969).

As for the presuppositions that the question makes about the practice of sociolinguistics current then, the broadest conclusion is that it was practised without a theoretical framework to guide and support its investigations. De Camp called it, after Kuhn, the fact-gathering or butterfly-collecting stage of sociolinguistics. However, dissatisfaction with mere fact-gathering was also becoming apparent and was finding progressive reflection in the works of Hymes and Labov, apart from De Camp's own paper. The different degrees to which concern with theory motivates these two important sociolinguists will form a major part of our later discussion. For the present, let it simply be noted that at this time the sociolinguist's freedom in searching for a theoretical paradigm of its own was considerably curtailed (or facilitated, depending on how you look at it) by the existence of a highly successful paradigm in linguistic theory, that of transformational generative grammar (TGG). There was of course the fact that TGG was, if anything, more structure-oriented than the theories of linguistics that had gone before, and it also seemed to share the distrust of language use and functional variety that had characterized the earlier theories. Furthermore, it appeared to be more theoretically oriented when compared to the theories of Bloomfield and the post-Bloomfieldians whose view of language was in most cases more 'empirical' at least in the sense that it was an idiolect and not competence that was being described. Therefore, De Camp was certainly being very perceptive in suggesting that, more than any other linguistic theory of the recent past, it was

the TG theory that showed the greatest promise of providing a place for sociolinguistics, and that the sociolinguists had better seize this chance since "sociolinguists simply cannot afford to continue its aimless drifting, collecting pretty butterflies and quaint word-lists in self-imposed isolation from the exciting things that are happening in general theoretical linguistics" (De Camp 1969:167).

The sociolinguist's (the term is used in the widest sense to include every serious student of language in its socio-cultural context) dissatisfaction with the prevailing linguistic theories was, however, too deep-seated to be overcome so easily. In 1968, talking of the linguist's concern with language as a formal discipline and of his ambition to raise it to the abstract level of mathematics, Fishman complained: "Psychologizing and sociologizing have not only been ignored but have been attacked in former years by the most distinguished American linguists as dangerous and misleading pursuits" (Fishman 1968:7). As a result, linguists with strong socio-cultural interests were forced out of the mainstream of linguistics and found shelter in anthropology where they set up a "parallel tradition" of linguistics which came to be known as "anthropological linguistics". This was particularly true of America where structuralism flourished in a purer form than in Europe, where the functionalist orientation of the Prague and London school of linguistics had always assumed a fairer deal to the socio-culturally inclined linguist.

What did these linguists seek in anthropology? Everything that they missed in linguistics: a socio-cultural context for language, a study of language in actual use in an endless variety of cultural contexts, a connection between language and other cultural forms of living, an 'empirical' methodology which would enable them to remain firmly rooted in the world of objective reality and save them from the abstractions of a 'theory'. In Great Britain, the London School led by J.R. Firth provided a centre of attraction for such linguists. Firth's linguistics, with its insistence on the study of language in "the context of situation" as defined by the anthropologist Malinowski, had almost come to be looked upon as a branch of anthropology (hence the description of Firth as "the anthropologists own linguist" (Ardener 1969) though Malinowski is yet to be described as "the linguists' own anthropologist"). In America, a strong tradition of linguistic anthropology had been in existence since the days of Boas and Sapir. In both the countries the dissatisfied linguist found much in these traditions to satisfy his interests and his new way of doing linguistics laid the foundations of anthropological linguistics.

At about the same time there were people in anthropology, specially in France, who were dissatisfied with the state of anthropology. Levi-Strauss, who expressed this dissatisfaction most strongly (Levi-Strauss 1963:Ch. 1) believed that anthropology suffered from a surfeit of empiricism. He rejected the atomistic and mechanistic interpretations of evolutionism and diffusionism as well as the naturalistic and empirical approach of British

functionalism. Thus, almost at the same time as the dissatisfied linguists turned to anthropology, Levi-Strauss turned to structural linguistics as a model of scientific method for anthropology. He attributed the achievements of linguistics to the structural method, which according to Troubetzkoy, its founder, was characterized by four basic 'operations': first, shifting from the study of conscious linguistic phenomena to their unconscious infrastructure; second, not treating terms as independent entities but rather taking as its basis of analysis the relations between the terms; third, introducing the concept of system, and finally the discovery of general laws, either by induction "or... by logical deduction, which would give them an absolute character" (Levi-Strauss 1963:33). As Levi-Strauss's influence in social anthropology grew and more and more social anthropologists began to follow his lead (e.g. Evans-Pritchard, Needham, Leach), their interest in linguistics also grew. As a result, the effort in anthropology to define a new methodology and a new approach to its subject matter began to gather momentum. Scientific rigour was now demanded of anthropological and sociological studies. Meanwhile, the progress of linguistics, which reached new heights in TGG, confirmed the soundness of the structuralist method and established its claims of scientific rigour. It must be noted here that, although TGG considers the application of this method by the post-Bloomfieldians narrow and somewhat misguided, it itself falls within the overall paradigm of structuralism. This is shown by the fact that TGG not only continues to operate with the four basic assumptions of structur-

alism as described by Troubetzkoy, but has in fact further strengthened and refined their application to the task of linguistic analysis, leading to an explanatory rather than a merely descriptive theory of language. This has enabled it to state the issues with greater clarity and precision, while the development of a logico-mathematical formalism has contributed greatly to its standards of scientific rigour which few among the social sciences can today claim to match. The acid test that Levi-Strauss set out for a scientific theory, that it be "real, simplifying, and explanatory" (op cit 35) has never been passed with greater success by any other theory of language. In fact, the recent progress in TG linguistics has been made possible precisely because of its obsessive concern with questions of reality, simplicity, and explanatory power. Linguistics has thus renewed its importance as a model of the application of the scientific method in the area of social science. Implicit recognition of this comes from the earnestness with which other social sciences have now started looking for explanatory theories in their areas, while explicit recognition now seems to come more often from the psychological end of the social science scale.

This has inevitably posed a problem for the dissatisfied linguists who turned to anthropology in search of a more 'empirical' methodology for linguistics. They must now come back to their parent discipline to learn more about methodology, if not about language. The transition is perhaps nowhere reflected so well as in the way in which the term <u>anthropological linguistics</u> has

gradually come to be replaced in the linguists' terminology by the term sociolinguistics. Following Malinowski's treatment of linguistics as "a branch of the general science of culture" (Malinowski 1937:172), anthropological linguistics had always been treated as a branch of cultural anthropology. But now the field seemed to divide itself among those in whose study priority was given to language and whose aim seemed to be the enlargement of linguistic theory by replacing the "somewhat antiseptic ideal speaker-hearer" with "sociolinguistic man and his many-sided competence" (Traciks 1969), and those in whose study priority was still given to "the whole organization of social behaviour" and by whom linguistic data were used only to identify and interpret other social phenomena (Edwards 1976:9). Sociolinguistics as the term for the former type of study has come to be regarded as the linguists' answer to the questions raised by the breakaway linguist and has provided the shelter he so badly needed. The latter study, termed sociology of language, has accommodated the anthropologists whose interest in language had always been extrinsic and instrumental. In choosing the term sociolinguistics, the former linguist now professes to be doing a kind of linguistics (may be, in his view, the only kind of linguistics) and not a kind of sociology or anthropology.

2

It is worthwhile asking what the reasons for the success of structuralism in linguistics have been. They are already

implicit in the four 'operations' (perhaps better called principles) of structuralism listed by Troubetzkoy. In more recent linguistics, it is the first and the fourth principle (shifting from the study of conscious phenomena to the unconscious infra-structure and the discovery of general laws) which have acquired greater prominence; the second (terms as relational entities) and third (the concept of system) now form part of the presuppositions of most linguistic theories and are no longer helpful in differentiating between them. Together the first and the fourth principle now lead one to the theory-building approach so well-known in the natural sciences and now becoming increasingly popular in the social sciences. The liberating influence of this approach can be attributed to its abandonment of the principle of radical empiricism which says that the constructs of the theory must be directly verifiable in the data. The theory, however, thereby does not cease to be empirical since the constraint of the overall verifiability (or falsifiability) of the theory is still in operation. Although weaker in nature than the radical empiricist constraint, it has been argued (in Chomsky 1957 and elsewhere) that it is not only the only feasible constraint to impose on a linguistic theory in our present state of knowledge but also the one most likely to lead to an insightful understanding of the subject of our study. This approach gives considerable freedom to the linguistic theorist to arrive at his theoretical constructs the way he chooses, from intuition, conjecture, and, importantly, not excluding empirical observation. By lifting the direct verifiability constraint, this

approach invites the theorist to propose bolder and more daring theories, frees him from an irritating and unrewarding preoccupation with the purity of his method, and enables him to concentrate on the insight-giving aspects of his analysis in place of purposeless cataloguing and classification. Such analyses must, of necessity, penetrate beneath the superficial and the consciously available reality to the underlying and unconscious infrastructure, if they are to be able to discover the general and universal principles that will account for the diverse and deceptive phenomena on the surface.

What is the nature of these general laws and how are they discovered? Levi-Strauss allowed for the possibility of an inductive discovery of such laws and seemed prepared to tolerate the non-absolute character which this would bestow on them. The notion of inductively discovered laws, however, consorts ill with a theory with claims to describe reality, for while reality is known with absolute certainty, inductively discovered laws are not. It is therefore not surprising to find the theory of TGG shifting to a deductive formulation of the general laws, for this alone can ensure that rules of grammar are known with absolute certainty and make sense of the claim that they are descriptions of reality. But, once again, this must not be interpreted as affecting the empirical character of the theory: the falsifiability criterion has not been discarded.

The combined force of the two basic principles of structuralism, enhanced further by the conceptual adjustments just

indicated, has resulted in a linguistic theory which is a powerful tool for understanding language. The opponents of this approach often interpret 'theory' to mean abstractions and idle speculation, having little or no bearing on reality, or a search for the elusive 'essences'. Although no longer so naively expressed, the attitude still seems to motivate much of the opposition to TGG. We therefore need to remind ourselves once again that theories are basically tools which man creates for understanding himself and his environment. As such they are to be distinguished from theories as "tools of prediction" which, as Popper showed (1963:113), are intolerable because they cannot be falsified. Theories as tools of understanding reality not only avoid the essentialist position but are basically falsifiable too. A given theory may make predictions which clash with reality; to that extent the theory fails as a tool and must be improved upon. The 'tool' character of theories lies essentially in what Popper called the "conjectural" and "hypothetical" character of theories. However, as Popper puts it, "it would be a grave mistake to conclude from this that the uncertainty of a theory, i.e. its hypothetical or conjectural character, diminishes in any way its implicit claim to describe something real" (op cit:116).

To return to structuralism and the study of language. One important illustration of the shift from conscious to unconscious phenomena in the study of language is the way in which the Saussurean notion of <u>langue</u>, which is concrete and cannot take into account abstract entities lying outside the speaker's awareness (Saussure 1959:138), has been raised to the abstract level of

Chomskian competence, which, almost by definition, deals "with mental processes that are far beyond the level of actual or even potential consciousness" (Chomsky 1965:8). It is an encouraging sign that some important sociolinguists like Hymes now accept this notion of competence and seek to widen its scope to include the communicative (rather than the merely linguistic) abilities of a speaker-hearer in a heterogeneous speech community, for it indicates that at least some sociolinguists are no longer averse to the theory-building approach in studying the socio-cultural dimensions of language. Not all sociolinguists, however, seem to agree that the need of the hour in sociolinguistics is a theory (e.g. Labov). The number of such linguists today is much smaller than, say, ten years ago when Ardener (1969) could divide the field into what he labelled Sociolinguistics A, denoting the structuralists, and Sociolinguistics B, denoting the functionalists, and reach the conclusion that "sociolinguistics as generally described is essentially a Sociolinguistics B" (1969:lxxvi). He included Hymes and Pride among the latter. I think Hymes has since then made his theoretical assumptions sufficiently clear (see, e.g. Hymes 1972) against any such inclusion, while what Ardener read in Pride "at his most analytical" is not borne out by Pride at his least theoretical. For example, in the same volume, Pride critizes Fishman for suggesting that analysis could progress "from descriptive adequacy alone to predictive power with respect to the what and the why of variety use" and comments on the lack of "what one might wish to call the study of sociolinguistic aspects of explanatory adequacy" (Pride 1969:Fn.6). Even Labov, although he is

very categorical in his declaration that, "I do not believe that we need at this point a new theory of language; rather, we need a new way of doing linguistics" (Labov 1972:259), agrees on "the fact that a purely descriptive approach does not really lead anywhere" and appreciates De Camp's concern for "a more abstract theoretical approach" (Alatis 1969:171). In actual fact, Labov makes calculated efforts in his analysis to incorporate rules of variation in the grammer itself, where grammar is conceived of in the Chomskian sense as a model of competence.

While the sociolinguist has thus gradually come round to accept the desirability of a theoretical framework within the overall structuralist paradigm for the study of sociolinguistic data, not much attention has been paid to the theoretical issues arising from the adoption of a specific framework, that of the successful TGG. And even less attention has of course been paid to the results of partial and convenient borrowings from that framework. As a result, misunderstandings both of the original framework and of its applications to the sociolinguistic data have multiplied, leading to the belief, in certain quarters, that TGG is basically incapable of dealing with any phenomena of empirical import (Derwing 1973) and that it is essentially a conceptual discipline like logic and mathematics (Itkonen 1975, 1976). In this section we shall try to show that these beliefs about the nature of TGG are quite mistaken, and, through an examination of the theoretical issues involved in the application of the TGG framework to sociolinguistic data, shall try to indicate the correct strategy for sociolinguistics.

It is not difficult to see the origin of these mistaken beliefs about TGG. Linguistic competence, as viewed by Chomsky, has an element of necessity since it is considered as the speaker-hearer's knowledge of the rules of his language. Competence is attributed to the native speaker-hearer since it is thought that without such competence the facts of his language would not become available to him. Inasmuch as the linguist's theory of a language, or its grammar, is a model of this competence, it is assumed that the linguist's methodology would also not permit him access to the relevant data until he has such a theory at his disposal. The same remarks would naturally apply to the sociolinguist's version of competence as well.

It is this necessary character of competence that has led to the reaction, common in form but diverse in intent, that TGG is not an empirical discipline at all (Derwing, Itkonen). The first of these writers subscribes to the strong version of the verifiability theory and we shall deal with his arguments later. But Itkonen's argument is fundamentally based on the necessary character of grammar. He classes grammar with logic and philosophical analysis: "The common denominator is the attempt to turn intuitive necessity into formal necessity" (Itkonen 1976:16). Itkonen calls this process the process of explication as distinct from explanation. He bases his argument on the fact that what a grammar accounts for is the notion of intuitive correctness of sentences and not any empirical notion of correctness. The corpus is irrelevant to the grammar since the speaker already has intuitive

recognition of correct sentences. In other words, he already has pre-theoretical knowledge of the language and what grammar does is to turn this intuitive understanding into a formally necessary algorithm about the language. Itkonen thus drives a wedge between the speaker-hearer's atheoretical knowledge of the rules of language and the linguist's theoretical description of them (Itkonen 1975:404). Only the atheoretical rules possess necessity since they are known with absolute certainty. Theoretical rules are the linguist's creation, as is shown by the fact that more and better rules are offered with the passage of time and "no grammar will ever be the true or the final one" (op cit). This distinction enables Itkonen to interpret grammar as an explication of the atheoretical, intuitively known rules, an axiomatization on the pattern of logic and mathematics. Conceived in this way, grammar is non-empirical since by definition its statements are not falsifiable by events in space and time; they deal only with intuitively known facts.

If Itkonen's atheoretical-theoretical distinction is accepted, we can no longer assume with Chomsky that what applies to the speaker-hearer applies automatically to the linguist as well. For example, we can no longer maintain that the theoretical construct 'competence' has a necessary character parallelling the necessary character of the speaker-hearer's atheoretical competence. This has important methodological consequences since the realist position is now replaced by the instrumentalist one. The axiomatization that the grammar represents is a formal device for predicting

the correct sentences of the language, but it no longer models the native speaker's competence and hence does not reflect the reality of language.

Itkonen then goes on to clarify that although grammar is non-empirical, it is not his view that all linguistics is non-empirical. Sociolinguistics and psycholinguistics "employ decidedly empirical methods" (1976:10). It is, however, not clear what Itkonen has an mind when he talks of sociolinguistics. The only sociolinguist of whom he talks at some length is Labov, and with regard to his variety of sociolinguistics his conclusion is that "sociolinguistics is no genuine alternative to grammar" and that "grammar could not possibly be replaced by sociolinguistics" (op cit:35). This conclusion is not surprising in view of Itkonen's conception of grammar as purely an axiomatization of atheoretically known rules. The point, however, is that it is possible to think of a sociolinguistics which could also be considered as an axiomatization of atheoretically known rules of linguistic appropriateness, feasibility, etc. There is nothing in the defining property of rules, as they are described by Itkonen, which can prevent us from thinking of the conventions which govern the use of language in cultural and social contexts as rules in his technical sense. They are known with absolute certainty, they control definite aspects of human behaviour, they determine an infinite number of correct actions, they are infalsifiable in exactly the same sense in which the rules of grammar are. We can match Itkonen's example of the definite article in English with any number of rules from

the sociolinguistic domain: the use of superior, inferior, and intimate forms of address, syntactic and phonological adjustments in formal, informal, and familiar styles, Labov's consonant cluster simplification, deletion of copula, negative concord, etc. in Black English Vernacular, and so on. The only arguable point here concerns the criterion of statisticalness (op cit:34), which we shall discuss below in the context of Labov's variable rules. It should, however, be clear from this overview that the shift in Itkonen's approach is mainly a methodological one, from the realist to the instrumentalist one, and that this says little about the substantive issues involved in the Chomskian notions of linguistic competence and grammar. Itkonen is of course at perfect liberty to treat grammar as axiomatization and sever its relations with questions of psychological reality, if an abstract study of grammar is what interests him. There is certainly a lot in grammar that can be studied abstractly and that can even aid understanding of language, but such severance prevents the linguist from saying anything about the nature of man's linguistic ability and its relationship with his cognitive capacities in general – questions towards the solution of which TGG is intended as the linguist's contribution.

Although Itkonen claims that his conception of linguistics is not very different from that of Labov, and that Labov conceives of the relation between grammar and sociolinguistics "roughly in the same way as I do" (1976:36), it is worthwhile examining the matter in some detail. Itkonen is perhaps led to make these

remarks on the basis of what Labov has to say about the invariant rules of grammar (Labov 1972:252), but he ignores the more basic difference between the concepts of competence and rule involved in the two views. Competence ceases to have any significance in Itkonen's system since it is no longer modelled by grammar, but Labov's attitude towards competence is an ambivalent one. He sometimes criticises the concentration on langue or competence and talks as if the notion of competence must be abandoned altogether (1972:191) and sometimes seeks to widen the notion of competence in the manner of Hymes (1972:226): "The ability of human beings to accept, preserve, and interpret rules with variable constraints is clearly an important aspect of their linguistic competence or langue". However, since Labov's competence is not available intuitively to the linguist and "only careful study of language in use" is supposed to reveal it to him, one is forced to the conclusion that Labov is trying to have the best of both kinds of linguistics without making the necessary theoretical adjustments which exclusive commitment to either type would demand of him. This is quite in consonance with his view that what we need today is a new way of doing linguistics by which we may be able to lend greater social realism to it. However, since it was quite open to him to make linguistics socially realistic without insisting on making rules a part of competence (a theory of performance also being part of an integrated theory of linguistic ability), Labov's refusal to do so must be interpreted as a reflection of his desire to assign a determining and necessary role to his variable rules

equivalent to the role played by the invariant rules. Statistical probabilities are thus raised to the level of certainties. Paradoxical though this may sound, it is not very different from saying that if it is possible to say that $R_1 \ldots R_n$ is a set of rules which predict that the organism O will behave in the manner M (the relation, once empirically established, being a necessary and determining one), then it should also be possible to say that given the invariant rules $R_1 \ldots R_n$ and the variable rules $VR_p \ldots VR_r$, there is a variable $\emptyset$ and probabilities $P_i \ldots P_k$ such that, in the presence of $\emptyset$, the collectivity C will behave in the manner M_1 in the order of probability P_i, in the manner M_2 in the order of probability P_j, and so on for every M and every P. Now of course it is possible to say this, but not for the organism O (hence not for the Chomskian competence) but for some sort of collective competence (Saussure's collective consciousness), for statistical probabilities here apply to collectivities and not individuals. For Labov's competence to make any sense, we must postulate a collective consciousness. Only in this interpretation the danger of being falsified at every turn can be avoided by the theory. Labov's linguistics may thus be seen as the culmination of the social aspect of Saussure's linguistic sign just as much as Chomskian linguistics is seen as the culmination of the psychological aspect of it.

However, for our purposes, the important conclusion that follows from the foregoing discussion is that, given the notion of a collective competence, sociolinguistics as practised by Labov

also assumes a non-empirical character and Itkonen's distinction between grammar and sociolinguistics (as non-empirical vs. empirical) disappears.

3

Hymes' notion of communicative competence illustrates how perfectly feasible it is to conceive of sociolinguistics as a comprehensive theory in which the socio-cultural context of a language can play "a constitutive and explicit role" in determining the communicative grammar of that language. Hymes starts with a three-way distinction between competence, ability for use, and performance. By assimilating ability for use to competence, he makes competence "a general term for the capabilities of a person" (Hymes 1971:282) divesting it of its purely cognitive character and including in it affective and volitive features as well. His expanded notion of competence is not synonymous with the formal possibilities of a system but also includes knowledge of feasibility, appropriateness and statistical probabilities (op cit:286). Despite this enlargement of the notion, however, Hymes does not diverge from the Chomskian notion of competence in any basic ways. Its locus is still in the individual. Hymes recognizes that "the relation between sentences and situations must be 'mentalistic', entailing a tacit knowledge, and hence competence (in the usage of both Chomsky and this paper)" (op cit:286). What he objects to in Chomsky is the restriction of competence to the grammatical and to the "implicit contradiction of a 'mentalistic' account of what

must in terms of the theory be a part of performance" – a contradiction which he tries to remove by splitting 'ability for use' from 'performance' and then assimilating it to competence, while at the same time helping "to place linguistic theory within a more general socio-cultural theory". As for the theoretical status of the notion of competence, he is in complete agreement with Chomsky: it is competence which makes the relevant data available to the child: "The acquisition of competence for use, indeed, can be stated in the same terms as acquisition of competence for grammar. Within the developmental matrix in which knowledge of the sentences of a language is acquired, children also acquire knowledge of a set of ways in which sentences are used. From a finite experience of speech acts and their interdependence with sociocultural features, they develop a general theory of the speaking appropriate in their community, which they employ, like other forms of tacit cultural knowledge (competence) in conducting and interpreting social life ... They come to be able to recognize, for example, appropriate and inappropriate interrogative behaviour...." (Hymes 1971:279). In view of this, it is perhaps too cautious on the part of Ardener (Ardener 1969:lxxvi) to class Hymes with sociolinguists of type B. His theoretical professions certainly seem to class Hymes with type A. In the same paper, Hymes almost echoes Levi-Strauss when he says, "the study of communicative competence ... may be called the ethnography of symbolic forms" and interprets "the recent shift in folklore studies and much of anthropology to the study of these genres (e.g. dance, drama, song, visual art, and various

forms of speech) in terms of performances with underlying rules" "as a reconstruction on an ethnographic basis of the vision expressed in Cassirer's philosophy of symbolic forms" and claims that this reconstruction is "already influenced in important part by linguistic study of competence" (Hymes 1971:284).

We are thus full circle in (socio) linguistics with what started as the linguist's desertion of linguistics for anthropology. The anthropologist wanting to import methods of linguistics has won out in the end, and our dissatisfied linguist has travelled the full circuit back home to linguistics.

4

What, then, was the fundamental error which misled our socioculturally inclined linguist into believing that the answer to his problems lay in the 'empirical' methodology of anthropology?

As I see it, the error lay in the view that the linguist was somehow being unempirical when he made the langue-parole or the competence-performance distinction, and promoted the study of langue or competence over that of parole or performance. What the critics failed to realize was not only that the competence-performance distinction did not exclude the empirical domain of psycho- and socio-cultural concomitants of language from the study of rule-governed behaviour (in fairness, we must admit that Saussure also did not realize this, witness his exclusion of sentence from langue on the plea that sentences are free creations, not rule-governed) and that an expanded notion of communicative

competence was a distinct possibility, but also that the TG study of grammar was far from being a non-empirical one. The two uses of the word 'empirical' – in the sense in which it refers to domains accessible to direct observation and in the sense in which it denotes a methodology of science – got intermingled in the minds of those who later on developed into the critics of TGG. This inability to keep the two meanings distinctly apart continues to plague a number of critics of TGG even today, and in discussing two of them (Itkonen and Derwing) we shall perhaps be able to see how much more difficult it must have been to make this distinction effectively operative in one's judgements a few decades ago.

Both Itkonen (1975, 1976) and Derwing (1973) seek to establish that TGG is non-empirical. Both of these argue that Chomsky's competence grammar is an axiomatization of the native speaker-hearer's knowledge and is isolated from linguistic performance. Their intentions of course do not coincide: whereas Derwing regards non-empiricalness of TGG a weakness and a reason for replacing it by empirical linguistics, Itkonen believes the non-empirical, or conceptual, character of TGG to be its strength which puts it on a part with logic, philosophy, and mathematics. However, their arguments seeking to establish the non-empiricalness of grammar show remarkable similarities. Grammar is not falsifiable by spatio-temporal data, says Itkonen. The axiomatization is not testable and is immune to empirical considerations, says Derwing: "a purely formal system which expresses nothing" (1973:286). Both assume the strong version of the testability criterion now generally

in disrepute in the methodology of natural and social sciences. Derwing makes it quite explicit (op cit:231) that, for the grammar to be testable, the concepts and constructs used in it must have direct interpretation in the data. Later (op cit:233) he tones it down somewhat and says that "before one can claim to have an 'empirical hypothesis' he must have isolated an empirical concept or two". If he were only suggesting, Labov-fashion, a new way of doing linguistics, we would have no quarrel with him, but his overall stand is in favour of a theory that "can be interpreted directly as a model of linguistic performance" (op cit:296), since the "abstractionist theory of TGG is (devoid of) all empirical significance" (op cit:286). For this to be true, we must assume the strong verifiability criterion, otherwise there would be no reason to doubt Chomsky's claim that "the actual data of linguistic performance will provide much evidence for determining the correctness of the hypotheses about underlying linguistic structure" (Chomsky 1965:18). Derwing tries to say that any interaction between a competence model and a performance model is ruled out by the logical incompatibility between the two as they are conceived by Chomsky (Derwing 1973:294). But Chomsky only talked of logical difference, not logical incompatibility (Chomsky 1971:188). Moreover, the link that Chomsky talks of between competence and performance is at the level of "actual practice", though he does not exclude a link at the methodological level (Chomsky 1965:18). (Obviously, for a methodological link to work, we must operate with Chomsky's individual notion of competence, not with a

collective notion such as we saw might be necessary to raise Labov's sociolinguistics to the level of a theory.) On the whole, however, there is nothing in Chomsky to suggest that no link exists, or can exist, between the competence and the performance components of the theory: what he suggests is that not much is currently known about the nature of this link. This can by no stretch of imagination be interpreted as a denial of a logical link between the two.

If this is so, then the insistence on 'empirical' in Derwing and Itkonen's work must be interpreted as insistence on direct incorporation of observed facts into the theory, and the entire intellectual effort which went into the development of a more abstract definition of scientific and empirical must be considered wasted. It would be wrong to consider this as a gain for the supposedly empirical domains of socio- or psycho-linguistics. For the crucial issue here is not whether grammar, sociolinguistics, etc. are to be called empirical or non-empirical, scientific or non-scientific; the crucial issue, as Chomsky and Levi-Strauss both point out in their different ways, is whether a particular approach will give us the insights that we need. That was the motivation for Levi-Strauss to look to linguistics for providing methods of analysis, and that was the motivation for Chomsky to question the objectivity-for-its-own-sake principle. This is of course not to say that the methodological issue can be brushed aside. Nor has it been brushed aside. If it had, other social sciences would not try to emulate linguistics, nor would

sociolinguistics eventually return to the linguistic fold. We must realize that, insofar as the ultimate goal of all theories is to explain the data, and insofar as data are unavailable in the absence of a theory, the nature of the data will not determine whether a theory is empirical or not, and scientific or not. Whether they are intuitions or observed facts, they must be melted down in the crucible of the theory, and only the final product will decide the issue.

The nature of sociolinguistic inquiry, therefore, cannot be very different from the nature of linguistic inquiry. In fact, both stand and fall together. What saved the linguistic inquiry from triviality is good enough to save the sociolinguistic inquiry too. To think otherwise is to mistake the superficial for the real, the contingent for the necessary, and idle speculation for theory.

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